







R E P L Y

TO

DR M<sup>c</sup>GILCHRIST'S "REMARKS"

ON

PROFESSOR BENNETT'S INTRODUCTORY LECTURE—

THE PRESENT STATE OF THE THEORY AND  
PRACTICE OF MEDICINE."

BY

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## REPLY, ETC.

*(Read to the Hunterian Medical Society, Feb. 8, 1856.)*

MR PRESIDENT AND GENTLEMEN,

IN accordance with your kind permission, I have this evening to solicit attention to a review of two pamphlets, which have recently been published, on the Present State of the Theory and Practice of Medicine. The first<sup>1</sup> is written by Professor Bennett, well known among us and, indeed, throughout Europe, for his scientific acquirements, for his earnest desire to devote these to the cause of practical medicine, and yet more for his skill in imbuing the minds of ingenuous youth with sound doctrine and professional ardour. The second pamphlet<sup>2</sup> is a criticism upon the former, written by John M'Gilchrist, M.D., a gentleman also known among us as being a member of the Hunterian Medical Society; moreover, if we read aright the hints thrown out by him in his pamphlet, he is so well acquainted with ancient medical authors as to be able often readily to find in them a parallel

<sup>1</sup> "The Present State of the Theory and Practice of Medicine." An Introductory Lecture to the Class of the Institutes of Medicine in the University of Edinburgh. Delivered November 6, 1855. By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and Clinical Medicine in the University. Edinburgh: Sutherland and Knox. 1855.

<sup>2</sup> Review of Professor Bennett's "Introductory Lecture," etc. By JOHN M'GILCHRIST, M.D. Edinburgh: Bell and Bradfute. 1856.

to novel modern theories [p. 21], and also so acute an analyzer as to be scarcely able to peruse a modern medical work, that professes to deal with principles, without encountering a lamentable logical confusion [p. 4]. In regard to these two pamphlets, I may first express my general opinion. The lecture of Dr Bennett appears singularly pleasing, from its hearty and hopeful tone, from the prominence of distinct ideas, the clear method by which each idea is in due succession developed, and the elegance of the illustrations no less correct in themselves than adapted to the audience before whom they were delivered. In the criticism of Dr M'Gilchrist will be found a painful contrast. It is disagreeable to the unbiassed from the unhealthiness of its tone, which betrays captious irritability against an individual professor, groundless despondency in regard to the advance of medical science, and undue depreciation of the advantages of Theory. It is disagreeable to the logician from the absence of prominent points and orderly arrangement; from the frequency with which premises are assumed, which Dr Bennett might reasonably dispute, and the manifest inconsequence between premises thus or otherwise assumed, and the conclusion deduced. To one who has carefully perused Dr Bennett's lecture it is disagreeable, from its misrepresentation of his views, incompetence to understand his arguments, and interpolation of fond theories. For those (and they are many), who have a respect for the author's talent and literary attainments, it is disagreeable to observe how he fails in redeeming his promise 'to indicate some of the specious fallacies, barren speculations, and *ignes fatui* of modern medicine.' Such fallacies and speculations do exist, and Dr M'Gilchrist would have benefited both Theory and Practice by distinct enumeration and exposure of some of them; but in coming to Dr Bennett's lecture, in search for them, he has been misguided by some will o' the wisp.

Such is my present judgment, and the justice of it will. I

hope, appear not only from your own careful perusal of both pamphlets, but also from the remarks which, with your permission, I shall now proceed to offer. They may be ranged under two divisions. The first contains a synopsis of Dr Bennett's lecture and refutation of Dr M'Gilchrist's expressed objections. The second contains an exposure of the mistakes which further recur in the more irrelevant portions of Dr M'Gilchrist's criticism.

Permit me then, first, to direct your attention to a synopsis of Dr Bennett's lecture and a refutation of Dr M'Gilchrist's objections.

To estimate the success of an effort, it is necessary to know the object it designs to accomplish. In the present instance, Dr Bennett's lecture is not designed to 'trumpet forth microscopic wonders to a generation of mankind asking for one grain of hope,' but to direct his own students to two points of interest, 1st, The relation of medical science to other branches of knowledge; 2d, The influential relation it has recently borne to the practice of medicine. According to the critic, its *professed* object is to set forth the practical triumphs of theoretical medicine over disease, but, as Dr Bennett himself explains his object to be twofold, we must regard the critic's representation as misrepresentation No. I.

In pursuance of his own purpose, and in regard to the first point of interest [the relation of medical science to other branches of knowledge], Dr Bennett shows (pp. 1-7), that medicine is related to several sciences by the common feature of complexity and imperfectness of generalization. The laws of medicine are many in number; each governs its own group of phenomena with deference to the influence of other laws; but, as yet, there has not been established one great law known to influence every vital phenomenon. But, secondly, under this head, Dr Bennett shows (pp. 7-9), that the advancement of

medical science is greatly furthered by the sciences of chemistry and of natural and moral philosophy. Having done this, Dr Bennett advances to his second object.

But what says the critic? First, he omits to state that this is one of the two objects of Dr Bennett's lecture; further, he omits all allusion to the beautiful illustrations adduced to show the connection of medical with other sciences (perhaps because they would have extorted praise even from him), and he restricts himself to an assault upon that classification which assigns medicine to the group of the inexact sciences. But this assault is a laughable manifestation of oscillating and feeble generalship. Instead of making one grand attack, and planting one heavy blow, by proving what is really in his heart to prove, that medicine is no science at all, he divides his forces and merely plays a siege. For, amid a cloud of words, there may be detected four accusations. *First*, That Dr Bennett is inconsistent.<sup>1</sup> How so? In three respects, says the critic. Because in many parts he speaks in the most reckless manner of medicine, as if it were an exact science. But let the critic adduce from among the many, one instance in proof; meanwhile I must consider it misrepresentation No. II. Dr Bennett is inconsistent again, because in many parts, in the most reckless manner, he speaks of medicine as if it were a mere art (*i.e.* I suppose an art without any science). If Dr Bennett so speaks, he is inconsistent; but, meanwhile, till proof is adduced, let this stand as misrepresentation No. III. Once more, Dr Bennett is inconsistent because he speaks of medicine now as a science, and again as an art. He does so, but in so doing, he acts consistently with

<sup>1</sup> "Dr Bennett begins by drawing a distinction between the exact and inexact sciences, and by assigning to medicine a place among the latter; but in many succeeding parts of his lecture, he loses sight of this important distinction, and in the most reckless manner subsequently speaks of medicine now as if it were an exact science,—again as a mere art."—(*Dr McGilchrist*, p. 4.)

common usage. We are all in the habit of saying, medicine cures and medicine teaches, just as we say, the art of logic and the science of logic, the musical art and the theory of music. But we are all wrong, and the critic will prove it<sup>1</sup> by two syllogisms, and certainly there is need of both. In the first, there is a major premise, viz., *the definition* [medicine is an inexact science] *is too general a definition*. The minor premise is wanting, and cannot by me be supplied, but the conclusion follows, nevertheless, that "to define medicine as an inexact science, and, in the next breath to speak of it as a healing art, seems [from this argument?] utterly inconsistent."

But syllogism second may prove this point; in it there is a major premise, viz., *the definition* [medicine is a healing art] *is nearly correct*. But again there is a hiatus—no minor premise occurs—and we are not necessarily driven to any conclusion. Thus, the argument, as it stands in Dr M'Gilchrist's pamphlet, has no logical force, from the absence of some important minor term, and that minor proposition is just the question at issue. May we not use the same word to indicate, now the science, and anon the art?

The second accusation is, that Dr Bennett has not proved his definition:—"Dr Bennett's first definition of medicine—that of 'inexact science'—will not bear analysis; for it would be necessary to show, of the separate branches of inquiry that make up the compound whole of medicine—that each of them separately is an inexact science, or that they all agree together in constituting such a science."—(*Dr M'Gilchrist*, p. 4.)

Now, in the first place, it is not necessary to SHOW anything of the kind, for a definition is not a demonstration: Euclid him-

<sup>1</sup> "To define medicine as an 'inexact science,' and, in the next breath, to speak of it as a 'healing art,' seems utterly inconsistent. For the first is much too general a definition, and will not bear analysis; the second is perhaps nearly correct; but if so, the scientific pretensions of medicine are at once disposed of."—(*Dr M'Gilchrist*, p. 4.)

self shows no reasons for his definition of a line, angle, or triangle. Further, the critic is wholly mistaken as to what is necessary for proof in the supposed case. He is stating the conditions necessary to prove a compound whole EXACT, but to prove it INEXACT, it is sufficient to prove or ascertain the inexactness of any one component part, and then to that extent at least the compound whole must also be inexact.

The third statement appears of a very obscure character terminating in the conclusion, that if something or other be granted as true in Dr Bennett's definition, a part will be greater than its whole.<sup>1</sup> To unravel the obscurity, two syllogisms are necessary. The first may be supplied thus, that chemistry is an exact science, and chemistry is one of the component parts of medicine, therefore one of the component parts of medicine is an exact science; and now the second syllogism advances to confound Dr Bennett by a *reductio ad absurdum*:—

One of the component parts of medicine is exact [says Dr Bennett]. But the compound whole is inexact [says Dr Bennett]. Therefore, says the critic, you must from these premises be drawn to the absurd conclusion, that *the part is greater than the whole*. By no means, the only logical and perfectly correct conclusion is, that the part is positive, and the whole negative (as oft-times we find in algebra), and so the part possesses an attribute which cannot be predicated of the whole, than which nothing is more common in nature.

And now the fourth and last assault, which should have been the only and the fully developed attack. The argument is designed to prove that medicine is in no sense a science: rather a difficult point to gain, but certainly one from which, if gained, he can

<sup>3</sup> But this is not the case, for, according to Dr Bennett, one of the branches that go to make up the so-called inexact science of medicine—chemistry, to wit—is itself an exact science; so that a part is greater than the whole, which is absurd.”—(*Dr M'Gilchrist*, p. 5).

sweep the whole position of the opponent. He advances his arguments<sup>1</sup> thus :—

1. Observation and experience are necessary for the existence of medicine. This is at once granted by Dr Bennett.

2. Observation and experience are in no sense sciences. This also is granted by Dr Bennett. BOTH PREMISES ARE GRANTED. What then has the critic to do, but to draw his own conclusion and win? Nay, but he must draw the logical conclusion, viz., that what are in no sense sciences, are yet indispensable for a science. Than which statement what is more true, and what more removed from the critic's own conclusion, viz., medicine is no science, because its indispensables are not sciences.

Thus, in the execution of the first half of his plan, Dr Bennett is wholly uninterrupted; the critic stumbles forward in four fruitless assaults, and exhibits at least four non-sequiturs in logic. Yet, this gentlemen can scarcely peruse a medical work, that professes to deal with principles, without encountering a lamentable logical confusion!

The second (or according to Dr M'Gilechrist), the professed object of Dr Bennett, was to show the influence of the science upon the art. This truth Dr Bennett illustrates most efficiently by two great and distinct lines of argument, viz., deductive and inductive. But prior to the development of either argument, there is necessary the establishment of a proper distinction between science and art. The main features of distinction, are accordingly delineated with a masterly hand, in the following words :—"We may consider science to be a collection of theories; art, a body of rules. Science says, this is or is not, this

<sup>1</sup> "Observation and experience are confessedly indispensable to the existence of medicine: but inasmuch as observation and experience are common alike to the savage and the physician, inasmuch as they are, in themselves, not sciences in any sense, *so it follows that*, in as far as medicine rests on observation and experience, *it is not in any sense a science.*"—(Dr M'Gilechrist, p. 5.)

is probable or improbable. Art says, do this, avoid that. The object of science is to discover facts and determine laws; the object of art is to accomplish an end, and determine the means of effecting it. Science is inductive and reasons; art is imitative, and exemplifies. Science is steady, certain, and progressive; art is vacillating, doubtful, and limited"—(*Dr Bennett*, p. 11.)

But again the critic advances to the attack, "In the above paragraph of definitions, the lecturer riots in a loose use of terms, the exact or positive, and the inexact or speculative sciences being confounded: what he means may or may not be true, but what he says is undeniably incorrect. Are the mathematics, for example, a collection of theories? Is astronomy, founded on mathematics, a collection of theories? Moreover, science is not necessarily inductive, nor does it necessarily reason; on the contrary, exact or positive science is deductive, and it does not reason, it demonstrates."—(*Dr M<sup>r</sup> Gilchrist*, p. 6.) In this criticism, there are four points of attack:—

1st, It appears to the critic, that in distinguishing between science and art, it is necessary to remember the subdivisions of science itself into exact and inexact, and not to confound them. It is by no means necessary; on the contrary, to give a proper distinction between science and art, it is advisable to confound the minor sub-divisions, and to delineate just those features which are in both contrasted to art.

2d, But is mathematics a collection of theories? Undoubtedly so in the honourable sense of the word. Hence, beside an innumerable number of Theoremata, which are just steps up the ladder to theory, we find such special names as "Theory of Parallels," "General Theory of Equations," "The Theory of Indices."

3d, Well, but is astronomy a collection of theories? Certainly—that word is also honourably known in astronomy. Thus the work, or one of the works, on which the recent calculations of Leverrier and Adams, were based, was the treatise of the French

astronomer Pontecoulant, and its title is, "The Analytical Theory of the System of the World." As to its use among English astronomers, turning over the pages of Sir John Herschel, I find the terms—Solar Theory, Lunar Theory, Elliptic Theory.<sup>1</sup> The last is by him declared to be "a true representation of nature." It was originally established by Kepler, but may be deduced from the yet wider generalization (also astronomical), the Theory of Universal Gravitation.

But 4th, We find that when Dr Bennett says, "Science is inductive and reasons" (*i.e.* is inductive and deductive), the critic sagaciously remarks, that science is not necessarily inductive, nor does it necessarily reason. Certainly not, for in regard to art, science may either infer or deduce. But the critic adds, "Exact science does not reason—it demonstrates." The distinction is futile—for exact science demonstrates by reasoning from certain premises to as certain a conclusion.

These four criticisms are quite ineffective, and the distinction between science and art must be admitted. And now, Dr Bennett is able to develop the argument by deduction, to prove that medical science influences medical art. It is substantially the following:—All sciences influence their respective arts. Medicine is a science; therefore, medicine influences its own art. —(*Dr Bennett*, pp. 11, 12.)

The major term is itself proved inductively by accumulation of instances, without regard to the now irrelevant distinction between exact and inexact: thus, the science of optics influences the art of constructing optical instruments—the science of mathematics influences astronomical observation—the science of electricity, the art of telegraphing—the science of astronomy, the art of navigation—and so all sciences in relation to their own art. The minor term—that medicine is a science—Dr Bennett assumes by consent of most well educated men, and the desired conclusion inevitably follows.

<sup>1</sup> *Vide* Herschel, p. 307, last edition.

How does the critic meet this argument? He might meet it by challenging and disproving the minor term: this he is well disposed to do, but no proof exists that medicine is no science except the miserable argument, that because the foundation is not science, therefore the superstructure is not science. To disprove the major term, the critic makes not one single effort; but while Dr Bennett is adducing the influence of science A on art A, of science B on art B, of science C on art C, the critic is triumphantly declaring that science A has not been proved to influence science D, "that the laws of optics refuse to be turned to physiological uses." But that question is *now* irrelevant—subsequently the critic's triumph will be questioned by me. Meanwhile, since neither premise is demolished, we deduce the conclusion, that medicine necessarily, as a science, influences its art.

But Dr Bennett is not content that this important position should be granted upon these general grounds, by deduction for the very nature of science. He proceeds to prove the same assertion, inductively by an accumulation of special instances.—(*Dr Bennett*, pp. 13–19.) Some of these are given at considerable length; to others he is content to allude as equally sure but demanding for their full development greater time, and probably greater knowledge in his audience, than he could claim. The list consists of the treatment of tænia and favus [not the treatment of scabies, as the critic would make us suppose], further, the management of tumours, abscesses, pneumonia, pleurisy, apoplexy, syphilis, small pox, phthisis, and Bright's disease. The list is large, and believing that each link in that chain is good, we are shut up to the conclusion, that medical theory is very influential upon the art.

In what manner does the critic deal with this argument? May I be permitted, with due deference to your judgment, to say that, in my opinion, he deals most unfairly. If Dr Bennett adopts this inductive method, what does fairness demand?

surely, that his whole list be examined before any cry be raised that the proof is inadequate, and the conclusion incompetent. If Sir Isaac Newton had attempted to show the influence of gravitation upon all bodies, and, for simplicity's sake, had begun with the homely fact, that a ripe apple, in a calm autumn night, falls to the ground—what would have been thought of a critic that cried, "Away with your ripe apple, your false analogies, your miserable comparison between things so different in kind as apples and planetary bodies, between results so disproportionate as a quiet bed at the bottom of a tree, and a ceaseless revolution through the immensity of space!" Yet, just so, Dr M'Gilchrist (after examining, very captiously, the first two simple instances) breaks forth into a contemptuous and expletive declamation, as if this man were not worthy to live! One excuse only can be offered: he thought he had listened to four instances, though he had only listened to two. Still, if as many, or more, remained behind, he was logically premature and unfair in ridiculing the conclusion to which Dr Bennett would bring us only by induction from ALL the instances. He ought with due candour to have weighed every argument.

Again I would ask, in what manner does the critic deal with this argument? and again I must answer, He trifles with the argument, and interrupts its course by interpolating theories about the lineal descent of syphilis from leprosy, and of cholera apparently from syphilis; by challenging Dr Bennett to come out of his course and argue the question of the change of types in pneumonia; and, by descanting upon the tendency of ancient theories to re-emerge in modern times. These speculations, coming from Dr M'Gilchrist, cannot be suspected of being barren—though, had they emanated from Dr Bennett, they would very probably have been pointed out as the latest *ignes fatui* in the marshes of theoretical medicine. True or false, these statements are irrelevant, and whatever their intrinsic value, the exhibition of them before Dr Bennett's argu-

ment was heard out fully to the end, must be regarded as a logical sin.

Let us, then, faithfully consider Dr Bennett's list *seriatim*, and then estimate the conclusion which he propounds.

The first instance is the application of science to the treatment of those afflicted by the *tænia solium*. The special department of medical science which is, in this treatment, applied, is human and comparative embryology; and the special truth in embryology which is directly applied is the curious fact, that the *tænia* and the *cystocercus* are two phases of one living creature. Dr Bennett does not wrench this fact out of its historical connection, as if it were an isolated, accidental, blind observation. He shows (and in a most interesting manner) by a series (as it were) of tableaux the successive stages in the development of the truth, and the gradual preparation of men, by one stage, to suspect, seek for, and comprehend the subsequent ones. The first stage is the preservation of the cloud of oyster ova in the water of Lake Fusaro, with a practical application for the increase of food. The second is the discovery that the ova of fish may be artificially impregnated and nursed, with a practical application again for the increase of food. Third, The curious investigations into the canine embryo, with this result only, that scientific men become deeply impressed with the conviction that different forms do not argue a difference in the living individual. Fourth, Investigating under this strong conviction, some scientific men discover that four so-called kinds of animals are but different forms in the history of one kind. Fifth, Then arises a suspicion that the *cystocercus* and the *tænia* are but two forms of one entozoon. Sixth, The suspicion is pushed on to proof by Kuchenmeister. Seventh and last stage in this eventful history is the practical application to disease.

At the conclusion of this *first* illustration, Dr Bennett remarks, that if the scientific facts of the first, second, or third

ages in the progress of these embryological discoveries had been alone revealed, we would not have seen their applicability to the cure of disease. To this assertion the critic himself bears ample testimony by confessing his own inability to discover it. Yet, through these stages, arrived the fifth and sixth stage, and thereupon embryology became a science applied in the detection and cure of disease. The same apparent barrenness, yet subsequent applicability, is of frequent occurrence in other sciences (it matters not whether exact or inexact). Even the greatest applications in this practical age may be traced from the ultimate result back to some trivial fact, interesting only to the scientific man. Thus, we know the steam-engine (in its present form) to be a result of Black's theory of latent heat; but had we been acquainted only with the theory, we would have counted it a mere dream—a barren speculation. Even so again, the electric telegraph was the result of the growth of a science whose first origin was but the spasmodic twitching of a galvanized frog. How few then foresaw the importance, practically, of that fact! So also, there are many truths in *medical* science which we may deem trifling, yet, in future ages, to these despised truths will be traced back most desirable improvements in medicine.

The greater portion of this elaborate instance is misapprehended by the critic: he believes that Dr Bennett is adducing the first steps in this argument to prove the application of scientific truth to medical practice, whereas he adduces them as but links in the advance of science from truth to truth onward to the final discovery of *tæniæ* and their formation—the conditions favourable for their development, and the conditions sufficient for their destruction: hence Dr M'Gilchrist's criticism about futile arguments must fall to the ground. When the true medical application is announced, he does not himself deny that our modern practice is based upon these scientific inquiries; but, misapprehending the analogy between truths in natural philo-

sophy [pregnant with use, though once thought barren], and truths in medical science [also pregnant with use, though once, and even now by some, thought barren], he ventures on a criticism, to the intent that it is wrong in Dr Bennett to rank the use of this medical truth as equal in importance to the use of new physical truths. The reply is ready. Dr Bennett makes no such direct comparison between uses and results: 1st, Because it is beside his purpose, which was to point out that practical inventions in our days may trace their origin to very unlikely truths; and 2d, Because he is too wise to estimate what is ever shifting in value. Thus, the electric telegraph is most important at this moment, for Government desires prompt information touching the state of the army. That information gained, the telegraph becomes of less importance and steam-engines rise in value, for troops require to be transported and telegraphs cannot do that. But neither telegraphs nor steam-engines will console a man who has tape-worm in his bowels. THE important thing for him is to get rid of the worm, and, further, to be warned how to avoid getting any more of their breed. The criticism is, therefore, a misrepresentation; and yet it is to hear this misrepresentation against an Edinburgh medical professor, that John Locke, of immortal memory, is raised from the dead. We are warned by common sense not to awaken a man abruptly from a sound sleep—he is apt to be out of temper. If the same rule prevails in Pluto's realms, the reason of *John M'Gilchrist Locke's* snappishness of manner is explained:—otherwise, his temper must have altered its type [like pneumonia], but for the worse, since he practised in Oxford 200 years ago. Further, his intellect also must have changed its type for a weaker form, if, like Dr M'Gilchrist, he misapprehended a comparison of ratios for a comparison of the things themselves. If Dr M'Gilchrist be honest, as none can doubt, in his confession of incompetence to see the truth of the analogy, the reason lies in his general ignorance of the true nature of analogy. Thus he

tells us (page 12), that analogy is impossible in this instance, *because the things are different in kind*; but the body of man in the grave and the seed sown in the ground, are also different in kind, yet a beautiful and valid analogy has been drawn between them in illustration of the body's resurrection. Again, he tells us that analogy is impossible, *for the results are disproportionate in degree*. Is there, then, no analogy between the intellect of man and instinct in the brute creation, because their respective results are notably disproportionate in degree?

Hitherto but one criticism has been offered against this first instance, viz., that it is not equal in importance to the discovery of steam-engines. Now, Dr M'Gilechrist does not adduce any other objection in his own name, but he ventures to put another into the mouth of that injured man whom he has resurrectionized. John Locke is made to pledge his word (in behalf of John M'Gilechrist, M.D.), that the treatment in case of *tæniæ* was the same in his own day, and the day of Celsus, as now, even to the point of forbidding certain articles of diet. If the great logician really said so [Dr M'Gilechrist only asserts he might possibly have said], then from reverence for his authority, we believe: but taking off the mask from this ghost, and discovering behind it the features of our friend, we become more sceptical, and demand from him farther proof that the modern practice was in all points known to Celsus and Locke.

Dr Bennett's second instance is the cure of favus, as a vegetable mould, by constant exclusion of air from the sporules and thalli; and the vast improvement in point of gentle and successful treatment is, with great interest, unfolded. But the critic declares, that old women cure the same disease by very gentle practice, viz.—good soap and water. Now, query, is it really favus with thalli and sporules which these females treat—query, are they indeed successful in curing true favus—and, query, in what time are they successful? Dr M'Gilechrist evidently takes for granted that old women can diagnose true favus from eczemas

(in which belief he is very simple, and may stand alone). He does not state in what time, but he is sure they do cure it. On Dr Bennett's authority let the statement be denied. The critic himself is not over sanguine of success from the good old treatment, for he adduces another argument in a most unhappy manner. After all, he says, the favus-patients are only youths and young children. Who, in the world, cares for the diseases of little children! and, besides, it gets well of itself in time [*i.e.* by the time of puberty], so that three, five, or ten years of disfigurement, loathsomeness, and disease, from the growth of a vegetable parasite which may be killed in six weeks, are regarded by the critic as a comparative trifle, not worthy of scientific attention!

Dr Bennett's third instance is more compound, and, doubtless, in the critic's opinion, more dignified. But just at this point the critic forsakes him, and enters upon a cyclical course of his own. First, he adduces another instance, the treatment of scabies, which Dr Bennett does not during this argument bring forward as a proof, yet in such a manner that most readers would suppose Dr Bennett had adduced it as a proof of the influence of science upon art. Then holding up the treatment of favus which Dr Bennett adduces, and the treatment of scabies which Dr Bennett does not adduce, as the choice and only conceivable triumphs of theory, he breaks forth into a pæan over the miserable failures and pretensions of scientific medicine. The pæan is wholly premature, for Dr Bennett's list is not yet exhausted, and it sounds, moreover, far liker a transcript from some unknown spasmodic tragedy, than a cool estimate of the arguments of the Professor. Thence he diverges hither and thither, nor returns to Dr Bennett's third instance till he has referred to the doctrines of Alison on change of type in pneumonia—on the relative importance of this or that theory—on the true history of leprosy and its lineal representative in the present day—on the coming disease—on the impotence of theoretical medicine [but

only in the absence of theoretical knowledge]—on the impossibility of discovering the laws which regulate health and disease—on the certainty that these laws are irregular and fugitive—on the reasons which may be conceived to have led to the abandonment of blood-letting in pneumonia. Meanwhile Dr Bennett's third instance is unassailed. Now, surely even if the critic were triumphant (which, certainly, he is not) in denio-  
 lishing Dr Bennett's first and second instance, he is scarcely acting like a "canny" logical Scotchman, who would have made "siccar" by another home-thrust at the remaining proofs. Rather does he act like an impulsive barbarian horde, which, supposing it has routed one flank of the opposing front, there-upon deems itself victorious, and turns at once to plunder the baggage.

At last Dr M'Gilechrist collects his forces and faces the third instance. In it Dr Bennett shows that the theory of cell-formation and its conditions (though not THE PRIMITIVE FACT which some suppose), is most useful already in application to treatment. What says the critic? Does he deny that it is a theory? No. That it is useful in practice? No. Yet *that* is the sole point of interest *now*. What then says the critic? He asks if Dr Bennett means to exhume a mouldering humoral pathology? The inquiry is palpably absurd to one who knows the vast gulph between the modern doctrines of cell development and the crude opinions of humoral pathology in former ages. But as the question is further irrelevant, it may be at once, on this ground, set aside; while *there*, unanswered and triumphant, stands Dr Bennett's third instance. From that point unassailed any more by critic, Dr Bennett rehearses his list of the treatment of growths, abscesses, pneumonia, pleurisy, and cancer, and claims as proper triumphs the beneficial changes in the treatment of apoplexy, syphilis, small-pox, phthisis, and Bright's disease. Therefrom, justly, he infers by induction that great is the influence of medical science.

But that influence, he acknowledges, is imperfect and capable of indefinite increase. One reason is obvious: The science itself is inexact; all vital phenomena are not yet reduced to law, and our interference with such phenomena cannot be scientific. While the main reason is that practical men, whose business and in whose power chiefly it is to apply truth, are not acquainted, as they might be, with the laws already determined, and do not make use, as they might do, of the appliances which require to be employed during the application of these laws. Thus, "notwithstanding the universality with which the stethoscope and auscultation are now received as necessary means of diagnosis, how few of our medical men, comparatively, are really skilful in detecting by them the morbid changes going on in the heart and lungs."—(*Dr Bennett*, p. 19.) In regard to this explanation of the limited influence of science we again confront the critic. For, when Dr Bennett declares that few, comparatively, are really skilful, Dr M'Gilehrst remarks that they never were more numerous; and whereas Dr Bennett states there are FEW really skilful, Dr M'Gilehrst, as if in proof, quotes Dr Henderson's authority to show, that *of these few, some are known to have occasionally failed*. Further, the critic states that as to the use of the stethoscope and microscope all medical men are agreed, yet he talks of thrusting a microscope into the flap of a wound, as if that were the ordinary and approved method of using the instrument. But supposing we be all agreed as to the mode of using them (which side up, and which screw to turn), what has this statement to do with the question, if we are all skilful in detecting by them the morbid changes going on in the heart and lungs? A clever mechanic may be taught the use of them—granting it—and granting that medical students are also taught the use of them, still there remains the question, do practitioners use them as it is within their power to do?

Finally, Dr Bennett looks to the future, and draws a bright

picture of the advance of science in itself, and in its application by practical men. Widely different is the language of the critic. "That science shall much avail to conquer disease, is the dream of the enthusiast, or the specious tale of the march-of-progress charlatan. . . . That disease with its changeful cycles, its startling revolutions, should ever stoop to the government of fixed laws, is as if the winds of heaven should again be confined in the cave of *Æolus*."—(*Dr M'Gilchrist*, p. 26.) Surely to the ear of students doomed by academie law to four long years of scientific study, on the hypothesis that it will much avail them, this doleful prophecy must sound painfully as the voice of despair, unless they be acquainted with varieties in human nature, and the tendency in some men to premature despondency and gloomy, though poetic, vaticination. Yet there are two grounds adduced for despair. 1st, The millennium is inconceivable:—"That medicine should ever become an exact science, is as if one language should become common to all the tribes of earth."—(*P.* 26.) No argument can be more sorry. Two hundred years ago, the timid astronomer would have declared that for the moon, with its changeful cycles, its startling revolutions, to submit to the government of fixed laws, was as if the winds of heaven should again be confined to the cave of *Æolus*. Yet this inconceivability has been fully realized. The argument is moreover ill-timed in the present age, when the rapidity of locomotion, and the establishment of telegraphs, and the prosperity of the country under free-trade, and the present alliance between France and England render us familiar with the fact that conceivability is no measure of truth. The second ground is more logical, and depends upon a division of natural laws by *Dr M'Gilchrist* (p. 18), into two groups. The first are regular and uniform in their operation: the second are indefinite, changeful, and fugitive:—the former are capable of being known, the latter are incapable of being known, and to the latter must be

referred the laws which govern health and disease. I object, on two grounds, to this statement; 1st, The division of laws into regular and irregular may be denied. All laws in nature are regular and uniform; that is the very meaning attached by science to the word law. LAW is the expression by the human intellect of the regular and constant as detected by it in what, to the mere sense of man, appears irregular and inconstant. Natural phenomena never are irregular and inconstant essentially; though they must appear so to the sense, till the intellect discovers the hidden order of their occurrence. But, 2dly, If the supposed group of laws, changeful, indefinite, and fugitive, be incapable of discovery (as Dr M'Gilechrist asserts), then the laws which govern health and disease cannot all belong to that group, for already some of them are discovered. Thus the regular preponderance of tubercle at the apex, of pneumonia at the base, of the lung,—the regular preponderance of serofula in youth, and of cancer in middle age,—the regular preference of certain diseases for certain periods of the year, and for certain portions of the earth's surface—the regularity of fevers, the regular progress of pneumonia, and of other inflammations. Indeed, it would be wearisome to enumerate them all; sufficient for our purpose to know the fact, ignored by Dr M'Gilechrist, that some of the laws of Pathology are already fixed. Yes, Gentlemen, some are already fixed, but others lie yonder for us to discover. There is a good time coming, and even now, he who sows shall reap a harvest, not only from his own labours, but also for the accumulated toils of successive generations of scientific men. To quote the concluding sentence of Dr Bennett's pamphlet, with one slight alteration:—“Everything promises that before long a law of true harmony will be formed out of the discordant materials which surround us; and if *they* our predecessors have failed, to *us*, I trust, will belong the honour of building up a system of medicine which from its consistency, simplicity and truth, may at the same time

attract the confidence of the public, and command the respect of the scientific world."

It is unnecessary further to express our high opinion of Dr Bennett's Introductory Lecture. It is admirable for the sagacity with which the leading question of the day is singled out as a suitable subject for an opening address—for its enlarged views of the relations of medical science—for the elegance and interest of its illustrations—for its methodical arrangement and sound reasoning.

It would have been more pleasant here to close, but in justice to Dr Bennett, the painful duty yet remains (in the second division) to point out the further errors of Dr M'Gilechrist in those passages, which are aberrations from Dr Bennett's line of argument. Such aberration is in itself a grave fault for a critic to commit. Irrelevant statements, however, may be correct, but Dr M'Gilechrist's irrelevant statements are also incorrect. I shall adduce nine instances:—

1. "The laws of optics refuse to be turned to physiological uses." (P. 7.) This is a startling statement to one who has taken up Valentin's Physiology—or who remembers the application of optical laws to the construction of microscopes, and the ease with which microscopes are turned to physiological uses. But why not of use? Because here, says the critic, are two optico-physiological phenomena, not yet satisfactorily explained. Granting his premises (which, however, are easily disputed), I must marvel at the boldness, which would deduce so sweeping a conclusion from the assumption that two optico-physiological phenomena are not yet explained.

2. "The planet Neptune was calculated—it was not discovered by calculation." (P. 8.) Here, indeed, is an important distinction in which Dr M'Gilechrist's subtilty appears in confessed power. Unfortunately there are two objections possible; this distinction is not ordinarily recognized by men of less subtle

analysis, and 2d, it is so subtile that Dr M'Gilehrist himself forgets it in a foot-note to that very page, and talks like other men of Leverrier, the DISCOVERER of Neptune. How far in that passage by tacitly excluding the contested claims of our countryman, Mr Adams, to a share in the honour, he yields to an anti-English feeling, I shall leave you, Gentlemen, to consider. It may be only an oversight, but as another instance of the same apparent fondness for arbitration and partiality in the assignment of honour, I must allude to his decision (p. 14) against Dr Bennett's claims as the introducer into this country, of the cod liver oil treatment in phthisis. His oracular decision is two-fold: 1st, Dr Bennett must rank after or along with Dr Williams of London (probably as a punishment for having laboured for nine years before Dr Williams in advocating the treatment); and 2d, Both gentlemen must lose by comparison with the obscure country practitioners and sensible old women of our own sea coast villages, and also the sensible old women of the continent with whom it found acceptance, and a refuge when slighted by all the colleges. The latter opinion is so absurd that even the authority of the resurrectionist of John Loeke will not persuade many to join him in comparing merit so different in kind and disproportionate in degree. The former contemptuous decision against Dr Bennett's claim to be ranked as the first scientific promoter of cod-liver oil treatment must strike every man acquainted with the real facts as extremely unfair, and, moreover, as singularly deficient in good taste.

3. "In serious cases of seabies," Dr M'Gilehrist tells us (p. 15),—"we are obliged to fall back upon an old wife's remedy once more, and to accept, with as good a grace as possible the nasty empirical sulphur ointment." By no means; if Dr M'Gilehrist had only quoted Dr Bennett's statement fairly to the end, a very different conclusion would have appeared; for the passage runs thus, 'In chronic cases, however, the egg

remain, and hence other applications may be sometimes necessary, such as the Stavesacre ointment, which Bourguignon has shown to be most powerful in destroying them."

4. "Dr Bennett, in the face of historical evidence and living experience, has denied that fevers and inflammations have ever changed their type."—(P. 19.) This is inaccurate. Dr Bennett nowhere denies that *fevers* have changed their type.

5. Dr M'Gilchrist respectfully asks, "What led to the abandonment of blood-letting in pneumonia?" but, instead of waiting for an answer as one would have expected from a respectful inquirer, he appears goaded on by a longing to fix Dr Bennett between the two horns of a dilemma, and volunteers a reply of his own. (P. 19.) "Either, he says, it was blind experience, or a theoretical demonstration of the *modus operandi* of venesection. Let Dr Bennett take his choice." But really Dr Bennett must decline: for, in truth, that which led to the change (at least in Germany) was neither blind experience, nor a theoretical demonstration of the *modus operandi* of venesection, but the homœopathic theory. Believing in this theory, Dr Fleischmann of the Homœopathic Hospital, Vienna, treated all pneumonias by globules of the appropriate remedy, and his patients recovered more quickly and thoroughly than in the bleeding establishment. But a medical bystander [Dr Joseph Skoda], observing and admitting the fact of recovery, formed another theory, that the cause was not the homœopathic dose but the power of nature, in the absence of interference by bleeding. So he gathered cases, and tested the power of nature without globules and without bleeding, with this result, that his patients recovered as well as the homœopathic, and, like them, better than those subjected to venesection.

6. "Theoretical medicine will stand silent, or retreat appalled before the next visitation of cholera." (P. 16.) Here again is a useful prophecy; but it will prove false. Theoretical medicine will not stand silent. It has *already* spoken with such authority

as to procure the timely interference of the Board of Health, and with such remarkable success that the last visit, terrible as it appeared beforehand to the imagination of empirical practitioners, was stripped, on its arrival, of many horrors and reduced to definite proportions. Neither did theoretical medicine retreat appalled. *E contra*, scientific men were foremost in exposing themselves, and busiest in the toilsome labour of detecting the nature of the disease and the manner of its propagation. Nor have these labours been in vain; but into the full measure of their success it is not desirable now to enter.

7. "Dr Bennett talks much as a more famous physician did some 2000 years ago."—(P. 21). "A more famous physician!" The critic, from the admirable balance of his mental faculties was able, with confidence, to estimate the relative importance of medical and a physical fact.—(P. 13). In like manner, he was able to distinguish between the value of one medical theory and another (P. 17), and to discriminate between the comparative merits of Dr Bennett, Dr Williams, certain obscure country practitioners, and sensible old women (*note*, P. 14). He not only takes a loftier flight; places in mental balance the fame of the modern, and the fame of the ancient physician, and on this point also, gives us the benefit of his judgment. But, suppose that Dr Bennett does talk much as did a more ancient physician, does he talk falsely? No. Then if he speaks truly, is it to his demerit that wise men lived in former ages, and in another country, who advocated (according to the dim light of science in their day) the same important truths against the same empirical mal-practices. It does not detract from his merit, as it did not detract from the merit of Dalton. Democritus advocated the same doctrine of atoms which subsequently, being elaborated, gained for Dalton the name of the "Father of Modern Chemistry." Yet none would rob Dalton of his honours on the ground that he talked much as did a more famous philosopher more than 2000 years ago.

8. Cancer is a disease of the blood; the surgeon is aware of it—yet he is not guided by theory, he is guided by a symptom. Such is substantially the statement of Dr M'Gilchrist. —(P. 22.) Now that cancer is a disease of the blood, is not a fact, but a theory; and the surgeon, in deciding to extirpate, or not extirpate, is guided by that theory. Sometimes he is guided by it to despair. Thus he says; I know the disease *by these symptoms* to be thorough cancer—THOROUGH CANCER IS INCURABLE—I will not interfere.

9. "Dr Bennett has embraced the ancient humoral pathology."—(P. 22.) The statement is doubly incorrect. Dr Bennett has not embraced any humoral pathology as a pathology sufficient to explain all the phenomena of disease; and 2d, If he has embraced any humoral pathology as explaining some groups of phenomena in disease, it is not the exhumed nor exploded humoral pathology of the ancients, but the modern theory, founded on more extended and accurate observations. The name appears to deceive the critic; because both are "humoral," he thinks that to embrace the one is to embrace the other; though the one is the first meagre generalization, and the other is the last full and scientific elucidation. Vast is the distance between the amount of knowledge within our reach, and the amount of knowledge which the ancients could apply. But of this great advance the critic takes no proper notice. He has a theory indeed [no mere *ignis fatui*, however, no barren speculation], that medicine moves not in a straight line, but in circles—apparently of the same diameter, without real progression. Surely the advance of medicine would be more correctly compared to a mighty stream. There are waves upon its surface, now sinking, and now rising, but there is, withal, a definite onward current, little affected by these minor and superficial changes. This doctrine, I think, is more in accordance with fact; and it is certainly more cheering than the belief that we are not moving on, but are merely deceived while moving round.

Thus the task is concluded imperfectly I must acknowledge, and with abundant roughnesses in style; but, in the circumstances, it is as perfectly concluded, as time and other engagements would admit. It is now my duty to listen to your faithful and ingenious criticism; in conclusion, allow me to express my thanks for your patient attention.

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